Will Conservation Savings Persist Over Time?

A Discussion Paper

for the
Bonneville Power Administration

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April 1992

CONTENTS

1. INTRODUCTION

1.1 Purpose

This paper discusses the persistence of conservation savings over time. Programs that deliver similar conservation savings year after year are more reliable than programs whose savings decline from one year to the next. The reliability of the conservation resource is an important issue for resource planners at the Bonneville Power Administration.

The illustration makes use of a computer simulation model implemented with the I-THINK software. This software facilitates easy visualization of the model structure on a Macintosh computer. The model represents the investment in efficiency measures by participants as well as nonparticipants in a conservation program to reduce electricity consumption in space heating in existing homes in the Northwest. The I-THINK model was constructed by adapting the residential space heating sector of **The Screening Model** used by Bonneville for the Macintosh illustration.

This is a preliminary report, and the analysis is illustrative and suggestive. Further research will be necessary to follow through on the suggestions. The goal of the follow-on research is to help resource planners interpret the results from program evaluation studies. A secondary goal is to demonstrate how conservation evaluations may be "pre-tested" prior to entering the expensive phase of the evaluation.

1.2. Background

Bonneville's evaluations of conservation resources are based mainly on short-term, sampled data with an occasional longitudinal study. These studies provide the principal means for Bonneville and the region to judge the long-term efficacy and cost-effectiveness of demand-side resources. This judgement is made difficult by at least four factors:

(1) limited measurements are available from the field;

(2) the data is quite complex, especially compared to measuring the electric output of a power station;

(3) the highly aggregated measures of conservation performance do not reveal the underlying causal factors; and

(4) planners still do not agree on the proper way to measure conservation performance.

Even though there is considerable diversity in the sampled data on performance, resource planners at Bonneville and the

Council typically rely on point estimates or averages. This approach allows the planners to proceed with their analyses of alternative commitments to conservation. But the simplified approach does not make full use of the information available from the evaluation studies. Also, the short-run observations of behavior do not preclude substantial changes in performance over the long run. Perhaps an equally important use of the evaluation studies is as an "early warning system." The studies may serve to alert resource planners on important changes by electricity consumers--both those who have participated in a program and those who have not.

The report begins with a short review of the relevant literature. I begin with references to the key evaluation studies that have been conducted over the past decade. I then note the reports where related topics have appeared in analyses with CPAM. The review concludes with references to "synthetic data experiments." These experiments combine the information from a simulation model with the design of a possible evaluation. The idea is to alert the evaluator to potential sources of distortion before detailed and expensive data collection begins.

2. REVIEW OF CONSERVATION EVALUATION STUDIES

Table I lists 11 evaluation reports on that have been reviewed in this study. These reports were selected from the many studies of conservation because they concentrate on the Northwest and they focus on the question of whether conservation savings will persist over time.

The first report was published around ten years ago by the Electric Power Research Institute. It describes a workshop to develop better methods to measure the impact of conservation and other demand side management programs. Interestingly, the Pacific Northwest utilities constituted the largest block of, workshop participants. The workshop discussions (p. vi) "indicated that the utility industry (had) made substantial progress in the last several years in measuring the impact of residential conservation programs." The discussants identified a variety of problems making further progress difficult. These include the proper level of aggregation and avoiding self-selection bias. At the top of the list of problems was "double counting." That is (p. vii) "how is it possible to isolate the effects of conservation programs on customer electricity use from the effects of weather, price, income and other "causal" variables?"

The second study in Table I is a critical review of conservation evaluations in the Pacific Northwest. The reviewers noted that (p. 21):

Table I. List of Conservation Evaluation Studies.

July 1982
Battelle Columbus Laboratories, "Workshop Proceedings: Measuring the Effects of Conservation Programs," report EA-2496 to the Electric Power Research Institute.

undated, probably around 1982
Linda Berry and Kim-Elaine Johnson, "Evaluations of Utility Residential Conservation Programs in the Pacific Northwest: A Critical Review," research sponsored by the BPA Office of Conservation, performed at the Energy Division of the Oak Ridge National Laboratory.

1984
Larry Condelli, Dane Archer, Elliot Aronson, Barbara Curbow, Beverly McLeod, Thomas Pettigrew, Lawrence White and Suzanne Yates, "Improving Utility Conservation Programs: Outcomes, Interventions and Evaluations," Energy, Vol. 9, No. 6, pp. 485-494.

September 1988
M. Hossein Haeri, "Electricity Savings Three Years After Participation in the Bonneville Power Administration Regionwide Weatherization Program," report ERC/PO-34 of ERC International, Portland, Oregon.

August 1989
Oak Ridge National Laboratory, "Electricity Savings One and Two Years After Weatherization: A Study of 1986 Participants in Bonneville's Residential Weatherization Program," report ORNL/CON-289.

1989
Pamela Brandis and M. Hossein Haeri, "The Persistence of Energy Savings Over Time: Two and Three Years After Participation in a Retrofit Program," proceedings of the 1989 Energy Program Evaluation Conference, Chicago."

September 1990
Oak Ridge National Laboratory, "Electricity Savings Among Participants Three Years After Weatherization in Bonneville's 1986 Residential Weatherization Program," report ORNL/CON-305.

June 1991
ERC International, "Long-Term impacts of the Interim Residential Weatherization Program on Household Energy Savings," report ERCE/DSM-65.

1991
Steven Nadel and Kenneth Keating, "Engineering. Estimates vs. Impact Evaluation Results: How Do They Compare and Why?" available from the American Council for an Energy Efficient Economy, 1001 Conn. Av NW, Suite 801, Washington DC 20036.

December 1991
Oak Ridge National Laboratory, "Handbook of Evaluation of Utility DSM Programs," report ORNL/CON-336.

March 1992 draft paper
Edward Vine, "Persistence of Energy Savings: What Do We Know and How Can It Be Ensured?" working paper available from Ed Vine, Lawrence Berkeley Laboratory, Building 90-4000, Berkeley, CA 94720.

as a whole, the evaluations suggest a need for further methodological development in the area of estimating energy savings attributable to programs. Most of the studies adjusted only for the confounding effects of weather. It is clear that other potential sources of bias must be examined too. Disentangling the confounding effects of factors such as weather, fuel prices, and differences between participant and nonparticipant groups is as difficult as it is important in unambiguously estimating the energy savings for a particular program, net of all other effects.

Berry and Johnston were impressed by the "early-late participant group design" in the home energy audit program by Seattle City Light. They argued (p. 21) that "the SCL HEC study probably produced the most reasonable estimate of program energy savings impacts. " They went on to recommend the SCL approach for future evaluation efforts, provided "it was supplemented with some form of multiple regression analysis. Berry and Johnson concluded their review by reminding the resource planners about the need for accurate conservation savings to use in capacity expansion models (p. 22):

the estimates of how much energy savings are attributable to the program must be accurate if the Net Present Value (NPV) of the program is to be calculated correctly. Capacity planning models that can determine the value of given amounts of conservation program savings relative to supply alternatives also provide essential input to NPV analysis. These models generally involve computer simulations that plan the generation requirements and costs of a range of forecasted demand levels. With these models the value of conservation program savings are quantified with respect to variations in future load growth, expansion plans and projected finances and rates. Thus, the correctness of NPV estimates depends on many assumptions and on the validity of the results obtained from other analytical efforts. Careful attention to the quality of these inputs to NPV analyses is necessary if one is to have confidence in the results.

The third study in Table I focuses on evaluation programs by California utilities. The paper concentrates on program implementation, especially the diffusion of innovation. The authors repeatedly criticize the California companies for their "marketing approach" to conservation. They emphasize (p. 489) that "advertising itself, at least as currently implemented by the utilities, is an ineffective way to change conservation related attitudes." They concluded with three "guidelines" for conservation planners (p. 493):

1. use energy consumption (not efficiency) as the measure of program impact;

2. discontinue advertising and concentrate on other ways to spread the word on the merits of conservation; and

3. rely on "hard interventions" such as direct provision of hardware to the customers.

The California study is less important than the other studies in Table I because it does not concentrate on evaluation issues. Their claim (p. 489) that "there is little empirical evidence that the rebound effect exists" is probably the most relevant observation for this review.

The next set of five entries in Table I deal with Bonneville's Residential Weatherization Program (RWP) . The RWP studies include two reports by ERC International, two reports from the Oak Ridge National Laboratory (ORNL) and a summary of the collection of studies by Brandis and Haeri. This body of work is the most important for my review because it provides one of the few pictures of how conservation savings change over time.

The first picture emerged from the ORNL summary of conservation savings "two years after" program participation. The ORNL researchers concluded that (p. xiv) "for the RWP overall, savings experienced by 1986 participants were substantial." First year savings (expressed on a weighted average basis relative to energy use prior to the program) was found to be 11.8%. Second year savings was 10.6%. ORNL staff concluded (p. 55) that

> the drop in savings could indicate an increase in "take-back" behavior, whereby participants in a conservation program take advantage of the improved energy-efficiency of their structures by raising thermostat settings or otherwise increasing occupant comfort. Whether or not the observed decline in savings is a durable trend in long term RWP performance or a short term aberration cannot be determined from the available data. Because of the importance that such a downward trend would represent for program planners, energy savings for the 1986 cohort should be tracked for at least one additional year. It might also be useful to extend the evaluation of savings to 4 and 5 years after retrofit. At the same time, research should be undertaken to explain the observed levels of conservation decay.

The ERC/PO-34 report on RWP savings "three years after" extends the "moving picture" of program savings into a third year. In a report to ORNL, Haeri noted that (p. 24):

energy savings remained remarkably stable during the three post-retrofit years. The results of our analysis show that energy consumption declined as the result of the program from an average of 23,860 kWh/yr to 21,760 kWh/yr in the first year, and decreased further to about 21,300 kWh/yr in the third year. Nonparticipants, on the other hand, increased their annual consumption over all three post-program years .... Based on the samples analyzed in this evaluation, "net" program-induced savings amounted on the average, to 2610 kWh in the first year (12% of pre-program energy use) and remained stable at that level for the next two years."

The ERC/PO-34 report makes no attempt to predict whether one would expect conservation savings to remain constant over time. Thus, it is not clear why the observed savings are "remarkably stable." Nevertheless, Haeri seems confident that the observations are important (p. iii) : "with regard to the long run energy savings effects of the program, the findings of this evaluation provide ample evidence indicating the persistence of program-induced energy savings over the three-year study period."

Later, in September of 1990, the ORNL published their report on the RWP savings "three years after" weatherization. The ORNL account is similar, but not identical to the previous report by ERC (p. xiv):

> During the first year after weatherization, weighted net savings averaged 3,060 kWh, or 13% of the previous year' energy consumption. In the second post weatherization year, a weighted average of 2,112 kWh was saved, amounting to 9% of preweatherization energy use. In the third postweatherization year, a net weighted average of 2,140 kWh was saved, amounting to 9% of preweatherization electricity use.

The ORNL staff attempted to explain differences in savings between different utilities participating in the study. Average net electricity savings from 7 different utilities was arranged along side of three possible explanatory factors in Table 4.6. (The factors were average per capita income, recent changes in the electric rate, and a measure of heating load.) The ORNL staff examined the data and concluded that (P. 28) "Table 4.6 shows no obvious relationship between net savings and any of the independent variables."

The ORNL report "three years after" dramatized the importance of attrition in sample size in longitudinal studies. Their table 2.1 report of data attrition is particularly dramatic. The study began with around 1,500 worksheets for program participants. Around 1,000 of these worksheets were excluded because the participants had already participated in a different program. Another 50 were excluded due to unusual billing records (which might indicate a high vacancy rate or the use of supplemental fuels such as wood). This left the evaluators with around 450 useful worksheets. But as the study unfolded over time, the sample was reduced due to turnover in occupancy of the retrofitted houses. By the end of the third year, the evaluators had only 252 useful worksheets. The decline from around 450 to 250 worksheets is a total attrition of 55% over the four year period. The size of the control group declined my a similar amount (from around 1,300 to around 700 for an attrition of 51%).

The rapid decline in sample size introduces two problems. One is whether the sample 'is large enough to provide useful, statistical results. The second is whether the group that remains in the sample after several years provides an unbiased picture of the RWP participants. Examples of "attrition bias" can be found when two different studies arrive at different estimates of the savings to be observed in a specific postweatherization year. These surprising differences appear when one compares the ORNL report of the second year savings in their August 1989 study (see Table 4.6, page 39) with their September 1990 account of savings in the same year (see Table 4.6, page 28). For example, the August 1989 study found that Tacoma participants saved 19.3% in the second postweatherization year. But in September of 1990, the ORNL staff (working with a smaller sample) observed that Tacoma participants saved only 10.6% in the second year. Other comparisons between the Table 4.6 appearing in the two studies shows changes in the opposite directions. For example, savings by Idaho Falls participants were observed to increase from 7.4% to 11.5%; savings by Snohomish PUD participants were observed to increase from 2.7% to 5.6%.

The paper by Brandis and Haeri summarizes trends in energy savings in Bonneville's weatherization programs since 1980. They conclude that (p. 320):

> while the electricity savings appear. to have remained stable in the Long-term Program, results pertaining to earlier programs show evidence of decay. Other studies of conservation programs, for example, an analysis of Seattle City Light's 1982 residential retrofit program, have also documented variations in year to year post-program savings. With respect to Bonneville retrofit programs, the results concerning the stability of savings remain inconclusive. Fluctuations in post-program savings for most of these programs are mostly irregular and reveal no clear patterns. In order to draw any definitive conclusions regarding stability (or instability),of savings, longer series of longitudinal observations. will be needed. Given the importance of this issue, there is clearly a need to monitor energy savings in these programs for a few additional years.

The June 1991 ERC International report provides the longest "moving picture" of the savings from Bonneville's RWP. The report is the fourth in a series of evaluations on the Interim RWP. it concludes that (p. i) "despite occasional fluctuations, net annual energy savings due to the interim RWP have persisted over time." At first glance, this persistence conclusion appears unusual since Table 5 shows the change in net DNAC (Difference in Net weather Adjusted energy Consumption) at 1,982 kWh/year in 1984 but only 1,439 kWh/year in 1989. This 27% decline is not the main conclusion from the study, however. Rather, the authors note that 1989 was a year of unusually low energy consumption (see p. 11), so the last useful year in the study appears to be 1988. The 1988 NDAC is found to be 1,737 kWh/year, down only 12% from the value back in 1984. In addition to the conventional charts of energy savings over time, the ERC report introduces an alternative measure of the "stability" of energy savings. Their new concept involves a ranking of energy use into five groups. They then examine the interim RWP data to learn that (p. 25) "on average, relative rankings are stable -- households in the highest savings categories remain in high categories, and households in the lowest savings categories remain in low categories." The ERC report does not explain how this alternative measure of stability might be used by resource planners at Bonneville.

In their ACEEE paper on why evaluation studies yield different results from engineering studies, Nadel and Keating provide important information on both the RWP and the Hood River Project by Bonneville (p. 3):

> for the RWP, net savings ... were reduced because control group houses also implemented some measures due to significant rate increases during the period of analysis. (This finding does not apply to the Hood River Project, which eliminated the control group from the final analysis due to problems with the control group selected. It should be noted that initial analyses of the Hood River Project reported that customers tended to set their thermostats slightly higher after the weatherization than before, thereby "taking back" some of the savings in the form of improved comfort. However, subsequent detailed analysis found that thermostat settings following weatherization were essentially the same as pre-weatherization settings.) ... Impact evaluations of the BPA program also show some interesting trends in terms of the persistence of energy savings. For the BPA program, savings were measured for one, two and three years after weatherization. For example, for homes weatherized in 1986, impact evaluation results were 58% of engineering estimates in the first year after weatherization, but only 40% in the third year after weatherization (an average drop of approximately 15% per

> year.) This drop was due to weatherization measures wearing out and slow adoption of weatherization measures by program nonparticipants ... For homes weatherized in 1985 and earlier years, savings also declined in the second and third years after weatherization, with the decline ranging from less than 1 % per year to nearly 20% per year, depending on the cohort being analyzed.

It appears that Nadel and Keating are more willing to describe the underlying factors causing the decline in net savings from the RWP over time. Whereas the previous reports (from ERC International and ORNL) found no apparent connection between savings decline and "any of the independent variables," Nadel and Keating put the forth a plausible explanation based on a combination of two reasons:

(1) wearing out of measures in the participants' homes, and

(2) an increased investment in the targeted measures by nonparticipants as they responded to increases in the price of electricity.

The illustrative modeling presented later in this report will confirm the plausibility of this explanation.

The December 1991 "Handbook of Evaluation of Utility DSM Programs" edited by Eric Hirst and John Reed provides a detailed explanation of utility evaluation programs. The project sponsors (from the U.S. Department of Energy and the New York State Energy R&D authority) open the handbook with the assertion that "program evaluation has become a central issue in the world of utility integrated resource planning." The sponsors go on to argue that (p. viii) planners

> have come to recognize the many technical disciplines that must be employed to evaluate DSM programs. An analysis might start out based on the principles of utility load research to find out what happened, but a combination of engineering and statistical methods must be used to "triangulate" an estimate of what would have happened without the program.

Kenneth Keating's chapter on the "Persistence of Energy Savings" is the most relevant portion of the handbook for my review. He summarizes two recent studies over a 6 year time period as follows:

> Two long term studies of persistence used billing data from participants and a comparison group. Both involved residential retrofit programs in the Pacific Northwest, one by Seattle City Light and one by BPA. Each followed samples of participants and nonparticipants for six years, and both weather-adjusted electricity consumption with

> PRISM. Two results are evident: the trend in energy savings is downward, and the decline is erratic (ie, net savings vary from year to year). The average decline in savings from the last five years compared to the first year amounts to 7% for the' Seattle program and 21% for the BPA program. These studies indicate substantial persistence, but also some erosion of savings. The 1980s were characterized in the Northwest by unprecedented electricity price increases for the first half of the decade, followed by declining real prices. Much of the erosion of savings was caused by reduced consumption by the comparison groups, about 2000 kWh during the six years studied, primarily because of electricity price increases.

Keating goes on to discuss the attrition problem in the longitudinal studies. He notes that BPA samples suffered an attrition rate of 55%, and he explains how the Seattle study took a "retrospective approach" by looking backward over time before selecting a suitable set of participants. (The backward-looking approach may solve the problem of a vanishing small sample size, but it does not necessarily eliminate bias from selecting an unusual subgroup of the total population.) Keating suggests that traditional evaluation approaches might be supplemented by an alternative, modeling approach (p. 98):

> An, as yet-untried alternative is an econometric approach. A Dynamic econometric model that defines the relationships among electricity use and electricity prices, measures of economic activity, and program participation could be developed. This model could then be used to predict what consumption would be in future years if energy efficiency were held constant and the actual values of electricity prices and other explanatory variables were entered into the model. If the consumption observed in the out-year was higher than predicted, then the efficiency effects could be said to be eroding, and the effect could be quantified.

Keating concludes his chapter with a call for more serious effort: "it is now time to address persistence in earnest.”

The final entry in Table I is a draft version of a paper to be presented by Ed Vine at the 1992 ACEEE conservation conference. It is interesting to conclude with Vine's working paper because the 1992 ACEEE conference will be scheduled over a decade after the 1982 EPRI workshop on , "Measuring the Effects of Utility Conservation Programs." Despite the utilities' efforts over the past ten years, Vine cautions the reader that research on the persistence of energy savings is "in its infancy." He cites a recent report to the California Institute for Energy Efficiency (CIEE) which asserts that "the persistence of energy savings was noted as probably the single, largest, unanswered question in demand-side management." He goes on. to draw the following conclusions from the limited information that is available (p. 6):

> The limited information on energy savings from persistence studies has shown that DSM program participants have not tended to increase their energy use over time; however, it is also truce that the control group of non-participants have tended to lower their energy use - over time, As a result, the difference in energy use between the participant group and nonparticipant group narrows, and "net savings" is reduced. Nevertheless, preliminary results indicate that the potential for the durability of net program savings is very good. With more detailed follow up of these programs and analysis of subgroups, the reasons for the changes in gross and net . energy savings can be elucidated.

Taken as a group, the 11 reports in Table I indicate that ten years of efforts have left conservation planners unsure about the persistence of conservation savings. Several of the studies draw no definitive conclusions about whether savings will persist over time and about the underlying causal factors that influence persistence. It appears that several of the researchers are drawn to the conclusion that conservation savings may decline over time due to a combination of two factors. The first factor is the simple wearing out of measures in the participants' homes. The second is the price-induced investment in some of the same measures by nonparticipants.

After a decade of "mixed results," many utility planners will wonder whether evaluation studies should be cut back, especially regarding our expectations for what can be learned about customer behavior. Maybe the lesson from the past ten years is that the causal factors are so hopelessly intermingled that evaluators will still be generating "mixed results" at the end of the 19-90s. The prospect of disappointing results must be considered, especially in light of the need for concrete measurement of demand side programs’s impacts when setting financial incentives and conducting demand side bidding programs.

Taken as a group, the papers in Table I do not call for a scaling back of evaluation efforts. Rather, they call for a more ambitious effort during the 1990s. Keating's concluding remark in the persistence chapter in the handbook (p. 99) reflects the general sentiment: "It is now time to address persistence in earnest."

3. REVIEW OF CPAM REPORTS

CPAM stands for the Conservation Policy Analysis Model, a screening model developed for Bonneville in the early 1980s. CPAM was used extensively during the 1980s to help conservation policy makers examine a wide variety of proposals for the Northwest electric system. The original CPAM approach is now incorporated in **The Screening Model** used in the broader area of resource planning at Bonneville.

Table II. Selected Reports Using CPAM.

September 1984 Andrew Ford and Steve Harris, "A Simpler Method for Calculating the Cost of Conservation Subsidies for an Electric Utility," Energy Policy.
May 1985 Andrew Ford and Roger Naill, "Conservation Policy in the Pacific Northwest," Technical report to the Office of Conservation, Bonneville Power Administration.
February 1988 Andrew Ford and Jay Geinzer, "The Impact of Performance Standards on the Uncertainty of the Pacific Northwest Electric System: A Final Report on the Hypersens Analysis of CPAM,11 technical report to the Office of Conservation, Bonneville Power Administration.
March 1988 Julie Mannes and Jay Geinzer, "Secondary Effects of Programs," unpublished report to the Bonneville Power Administration on contract task 1-2 by Applied Energy Services Inc.

Table II lists four reports using CPAM. These reports are familiar to Bonneville staff who operate The **Screening Model.** They are mentioned here simply to remind the reader that conservation planners must grapple with the same troubling concepts that make conservation evaluation difficult.

The first entry in Table II is a September 1984 paper in Energy Policy. It explains how a simulation model could be used to analyze the costs of conservation subsidy programs. The model was designed to address the "double counting" issue with a particular focus on the possible redundancy between efficiency measures targeted by a utility subsidy program and measures that nonparticipants would eventually purchase on their own. The paper uses conservation cost curves and subsidy programs from northern California to show that utility subsidy programs could end up being extremely expensive due to high redundancy. The paper advises conservation planners to target subsidy programs at either low income households (with high discount rates) or moderately expensive measures in order to reduce the redundancy with price-induced conservation. This 1984 paper is important because the "Simpler Method for Calculating the Cost of Conservation Subsidies for an Electric Utility" is one of the building blocks for CPAM.

The most important report in Table II is the May 1985 technical report on "Conservation Policy in the Pacific Northwest." Chapter 8 of this long report uses the regional version of CPAM to examine how conservation savings might change over time. The report explains the "indicated savings" to be expected from a subsidy design and the reduction in savings that can occur due to (1) delays in participants signing up for the program and (2) redundancy with price-induced investments. In contrast to the September 1984 study for northern California, the May 1985 study for the Northwest suggests that price redundancy would be limited to around 30% over the long term planning horizon (p. 8-15). The CPAM analysis also looked at "secondary effects" which include the "rebound effect", "price feedback," “interfuel substitution" and demolition of the housing stock prior to the wearing out of the measures. Because of the integrated design of CPAM, the model is particularly suited for the analysis of the "price-feedback" effect. This effect arises from changes in rates that must be implemented to finance the conservation program (and to compensate for "lost sales"). The changes in electric rates lead, in turn, to further changes in the demand for electricity. The May 1985 estimate of the "price feedback" effect demonstrated that planners could ignore this secondary effect over the longer term (if the aluminum industry is not sensitive to the industrial firm rate).

The February 1988 study on the uncertainty of the Northwest electric system is relevant because of the treatment of both dual fuel and single fuel standards on new buildings. In one pass through the sensitivity analysis of the energy system, efficiency standards were imposed on all new buildings, regardless of whether they were heated by electricity or natural gas. in the second pass, the standards were assumed to apply only to electrically heated buildings. This pair of sensitivity studies allowed Bonneville staff to see the impact of customers' shifts from electricity to natural gas heating to avoid the higher, up-front costs of the electricity-only standards.

The final report in Table II is the March 1988 update on the "Secondary Effects" previously analyzed in the May 1985 Technical Report to Bonneville. The update was performed by Julie Mannes and Jay Geinzer, and the report was written for limited use by the Bonneville staff who are intimately familiar with CPAM. The goal was to extend the list of "secondary effects" and to make use of the sub-regional version of CPAM that had just become available. The update shows that the conservation programs would exhibit only 3% redundancy, down around ten-fold from the 30% estimate from May of 1985. The dramatic decline in redundancy was attributed to combination of factors. For example, the March 1988 update uses the 1988 expectations for growth in the region's economy, for the need for new generating resources and the likely change in ,electricity prices over time. But Mannes and Geinzer emphasize that the dramatic decline is also due to an important change in model structure as CPAM was altered from a regional to a subregional model. The move, to greater complexity in the representation of the region's utilities was accompanied by a simplification of the treatment of the utilities' customers. Customers were portrayed in the regional model by three income groups (low, medium and high income), and each income group had a different discount rate. In the subregional model, however, the three groups of customers were replaced with a single income group with a medium discount rate. Mannes and Geinzer emphasize that the selection of the single discount rate to be used by nonparticipants is the key parameter in the model's projection of price redundancy.

4. THE LITERATURE ON SYNTHETIC DATA EXPERIMENTS

Table III lists three papers on the use of synthetic data to guide evaluation efforts. The 1985 paper in the Journal of Policy Analysis and Management deals with the confusion arising in efforts to verify the success of safety programs at OSHA, the Occupational Health and Safety Administration. Regression studies of OSHA's factory inspection programs consistently revealed little success in reducing industrial injury rates. This finding conflicted with the case studies, and OSHA planners were left wondering which type of study to believe. McCaffrey observed that regression studies are frequently viewed as more convincing (p. 198):

> Most analysts are prepared to acknowledge that descriptive techniques such as case studies can capture some of the subtle dynamics and interactive effects that easily escape the regression model; but because analysts have no easy way of knowing whether the cases. are representative, they usually prefer to dismiss the conflicting conclusions as unrepresentative, perhaps even anomalous. The usual conclusion of such analysts is that when carefully executed regression analyses point to one conclusion and descriptive approaches to another, the regression results are the more valuable.

McCaffrey challenges this assumption and demonstrates how the conflict between the regression studies and the case studies might arise. His demonstration makes use of a dynamic simulation model which serves as a link between regression based results and hypotheses gleaned from the case materials. The illustrative model is quite simple. It assumes that OSHA inspections are successful

Table III. List of Synthetic Data Papers.

1985 David McCaffrey, David Andersen, Paul McCold, and Doa Kim "Modeling Complexity: Using Dynamic Simulation to Link Regression and Case Studies," Journal of Policy Analysis and Management, Vol 4, No. 2, pages 196-216.
Summer 1989 Catherine Crawford, David Andersen and George Richardson, "Synthetic Data Experiments Using System Dynamics Models: A Survey of Results ' and a Research Agenda," System Dynamics Review, Vol. 5 No. 2, Summer 1989J, pages 199-208.
winter 1991 Catherine Crawford, "Endogenous Safety Processes: A Model of Regulation and Safety in Industrial Firms," System Dynamics Review, Vol. 7, No. 1, Winter 1991, pages 20-40.

in reducing injury rates. But it also assumes a link between the frequency of inspections and the degree to which accidents 'are reported. McCaffrey notes that (p. 204) "although the risk of such bias in the data is obvious, it has not been handled systematically in the statistical analyses so far." The model was used to generate "synthetic data" similar to the data collected in OSHA evaluations, and the researchers replicated the OSHA statistical analysis. This experiment revealed that (p. 212):

> When roughly 91% of accidents are assumed to have been reported prior to inspection, the regression model predicts no effect for OSHA, even though the simulation explicitly assumes an effect for OSHA in all cases. Where roughly 88-93% of accidents are assumed to have been reported prior to inspection, the OSHA effect is not statistically significant. For lower assumed reporting rates, the model predicts that OSHA inspections actually increase accidents--all this notwithstanding that the simulation has assigned a universally effective role for OSHA.

McCaffrey concludes that the important point from the illustration is (p. 212) that:

> experiments of this type can alert the analyst to potential sources of distortion in a study before detailed and expensive data collection begins. Also, such experiments can be used to probe the sensitivity of regression-based results to complications suggested in the case studies. Thus, the simulation models provide a bridge between case studies and regression studies via synthetic data experiments.

The second paper in Table III provides a survey of synthetic data experiments in which the simulation model uses the System Dynamics approach. The survey covers experiments with models of oil exploration and discovery, growth in urban areas, as well as the safety of OSHA inspected factories. The survey begins with a explanation of a synthetic data experiment (p. 199):

> In general, we define synthetic data experiments as research projects that involve two computer based models, a data generating model and an estimation model. Typically, the data generating model is a simulation model (for our purposes, think of a system dynamics model) that is designed to represent some aspects of a real world system. Data are generated by this model under a variety of stochastic conditions (process error is fed into the model, output variables are observed with measurement error, and so on), and these data may be sampled in a variety of ways. For example, multiple simulati6ns may be used to create a cross-sectional sample, or a single run may be sampled through simulated time (at various sampling intervals) to create a
> ,longitudinal synthetic data set. once sampled, the synthetic data are then used as inputs to a statistical model that is used to estimate some aspect of the data generating model, such as important parameters Or elements of system structure. The key to these experiments is that the exact structure and parameters of the data generating model are known in advance. The ability of the estimation model to recapture features of the data generating model can be used as a more or less pure test of the ability of the statistical sampling and estimating techniques to recover accurately the known properties of a data generating system.

The 1989 paper goes on to recommend how future researchers should organize synthetic data experiments to carefully build our understanding. It explains five classes of research projects arranged in order of complexity. The simplest two classes, replication of a previously published evaluation study and experiments with longitudinal misspecification, are most relevant to the study of conservation evaluation programs.

The third paper in Table III is the most recent published example of a synthetic data experiment using a system dynamics model. It explores the OSHA safety inspections topic in more detail and provides the reader with a glimpse inside the type of simulation model which could be used to generate the synthetic data.

5. PURPOSE OF THE ILLUSTRATIVE MODEL

A system dynamics model of conservation investments by participants and a control group was developed for this discussion paper. The model focuses on the "net savings" observed by comparing the energy consumption of the participants with the energy consumption *of a control group. The model is used here to explore what sets of conditions can lead to a decline in observed savings over time.

6. FIRST DEMONSTRATION

Figure 1 shows a "map" of the internal structure of the first demonstration model. (This diagram appears automatically on the Macintosh monitor when opening the file with the demo model.) The mapping conventions are explained in the I-Think documentation. They may also be explained by Bonneville staff who have used I-Think in previous studies. (For an example, contact Erik Westman or Glen Gettemy at Bonneville for information on an I-THINK based analysis of "Diminishing Returns of Increasing Consumer Incentives for Conservation".) The "map" in Figure 1 shows the model variables and their interactions. The actual equations appear in an appendix to this paper.

The demo model is comprised of two sectors which are enclosed within the thick, solid lines in Figure 1. The upper sector represents the control group whose installation of conservation measures is governed by a behavioral discount rate and the electric rate. The discount rate and the expected measure life are combined to form the capital recovery factor and to determine the "justifiable costs" (measured in $/kw) that the average member of the control group is willing to spend on conservation measures. The "justifiable measures" (measured in kw/house) are found from a nonlinear table. After an "adjustment time" to make the necessary investments, the "installed measures" (also measured in kw/house) will follow the "justifiable measures. 11 The *conservation* "savings" are found by adjusting the number of "installed measures" by the "intensity factor," the ratio of the house's base use relative to the base use in 1980 at the start of the simulation.

The lower sector in Figure I shows the participants' investment in *conservation measures*. (The shaded variables, such as "actual use" by the control group, in the lower sector are called "ghosts." They are simply variables calculated in the other sector, and a ghost icon is used rather than bringing a solid line down from the top of the diagram.) The demo model assumes that the participants are similar to the control group until the program is implemented in 1982. After 1982, they will work to install the measures targeted by the program.

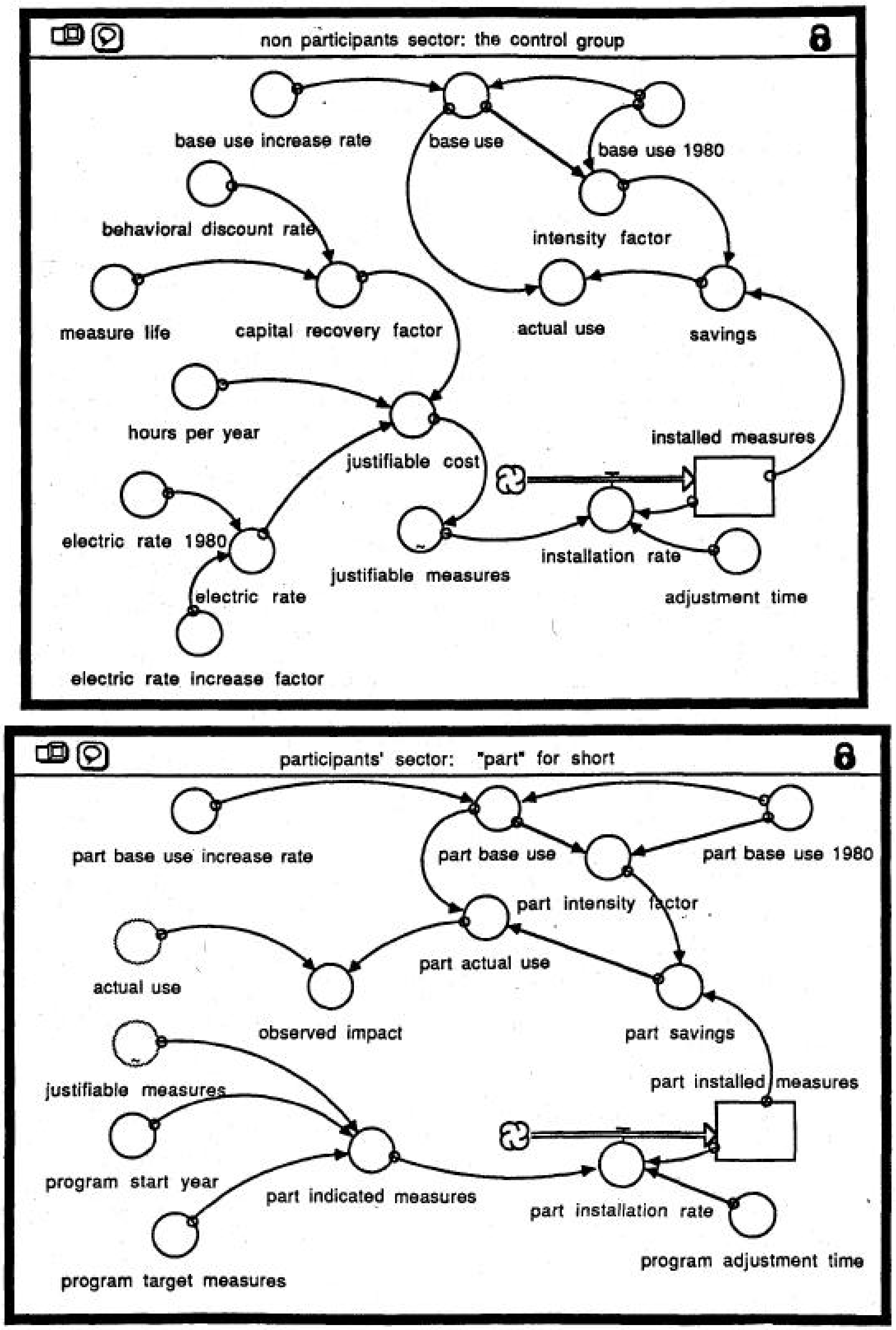


Figure 1. Structure of the First Demonstration Model

By comparing the "actual use" of electricity by the control group with the "participants actual use" of electricity, one obtains the "observed impact" of the program. The key question is whether the "observed impact" will decline over time.

Figure 2 shows the conservation savings curve that is used to move from "justifiable cost" to "justifiable measures" in the model. The extended caption explains the source for the curve and provides eight-examples of relevant points along the curve.

Figure 3 provides a "benchmark" projection from the demo model. The simulation begins in the year 1980 and ends in the year 1992. Since the conservation program does not start until 1982, the first two years of the simulation show the control group and the participants with the same energy use, and the "observed impact" is zero. The 1982 program targets all measures up to 0.63 kw/house which would cut the electricity use by 50%. This is an ambitious target that would end up costing around $1,200 per house.

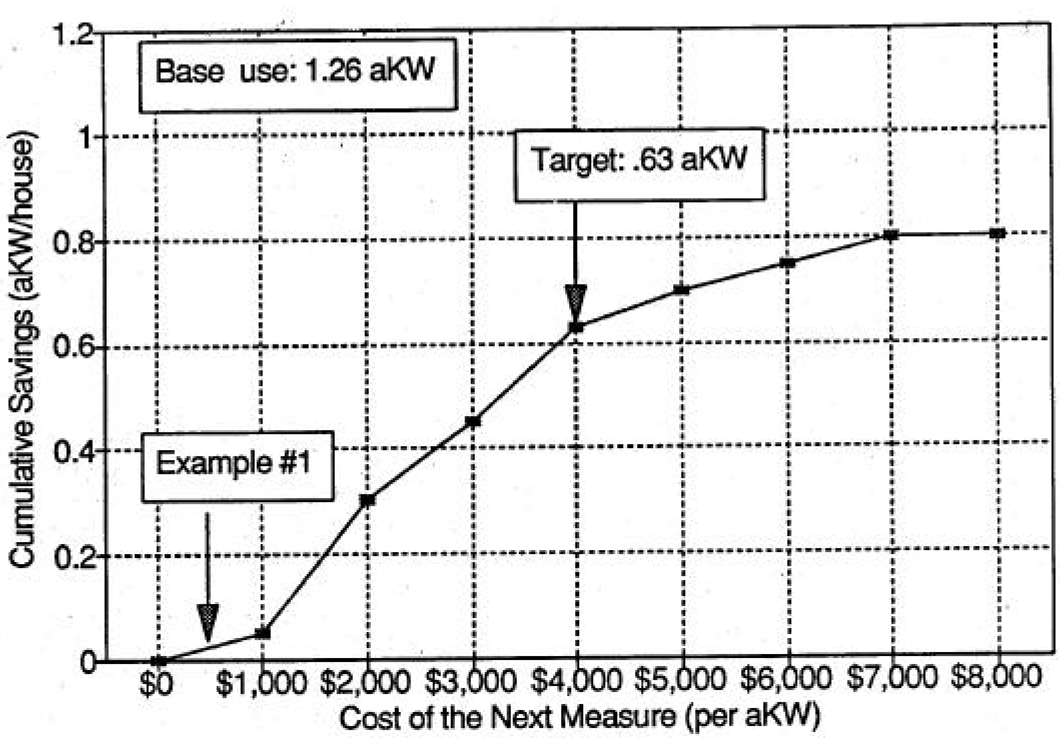
The control group is assumed to experience a constant electric rate of only 20 mills/kwhr. (The 1990 BPA demand forecast, Technical Appendix, pages 34-35 gives typical residential retail rates in 1981. The 20 mill/kwh rate is somewhat higher than the average residential. rate for public utilities in 1981.) The behavioral discount rate is set at 35%/year, a value midway between the discount rates employed in the medium low and medium high demand forecasts in the Council's 1986 Plan (seepage 4-8). As "Example 1" in Figure 2 notes, the average member of the control group would only be willing to spend around $500 per aKW with such low electric rates and such high discounting. Figure 2 shows that the "Example 1" position lies around 0.03 akw/house. In other words, one should expect the control group to remain essentially inactive during this simulation. Thus, the benchmark pattern in Figure 3 is quite simple:

1) electricity use by the control group remains essentially constant over time;

2) the participants' electricity use is cut approximately in half by the acquired measures; and

3) the observed impact increases quickly after the measures are installed and remains constant over the course of the ten year simulation.

Now we may experiment with the demo model to learn what might cause the observed impact to decline over time. Figure 4 shows an extreme example in which electric rates increase dramatically over the time period. For this example, the electric rate is assumed to increase at 11%/year in real terms. (This increase is similar to the increase in the average system



example	discount rate	capital recovery factor	electric rate (mills/kwh)	justifiable cost ($/aKW)
1	35%	0.350	20.0	501
2	20%	0.201	20.0	872
3	7%	0.081	20.0	2,174
4	35%	0.350	30.0	1,751
5	20%	0.201	30.0	1,308
6	7%	0.081	30.0	3,261
7	35%	0.350	27.7	692
8	35%	0.350	74.9	1,874

Figure 2. Conservation Savings Curve Used in the Demonstrations.

This curve is used to translate "Justifiable cost" into "justifiable measures" in the I-THINK demo modeling scheduled for May of 1992. The curve is approximately the same as the residential space heating cost curve shown in <u>Conservation Policy in the Pacific Northwest</u> (Ford and Naill 1985, p. 2-3). With a base use of 1.26 akw per house and a maximum possible savings of 0.8 aKW, the upper limit on savings due to efficiency improvements is 63%. The conservation program studied in the demo is assumed to capture all measures up to .63 aKW, for a savings of 50%. The cost of the measures is roughly the area of the triangle with a height of .6 aKW/house and a width of 4,000 $/aKW or approximately 1,200 $/house (in 1980 dollars).

The eight examples show some of the justifiable costs that will appear in the demonstration. The 1st example is noted directly on the chart. It assumes that the residential customers of a public utility pay 20 mills/kwh (in 1980 dollars). If their implicit discount rate were 35%/year, they could justify spending up to 501 $/aKW. This investment would only deliver around 0.03 aKW of savings. The next two examples show change in what the public utility customer could spend with changes in the implicit discount rate. Examples 4-6 are based on a residential rate of 30 mills/kwhr which is more typical of what the IOU customers paid back in the 1980s. The final two examples show the justifiable costs if, the 20 mill/kwh rate increases over time (to around 28 mills. in the 7th example or to around 75 mills in the 8th example).

cost reported in the 1983 Annual Report issued by Snohomish PUD.) The rapid increase in* the electric rate convinces the control group that they can justify spending up to 1,900 $/kw, and Figure 2 shows that this would cover measures saving up to 0.27 kw/house. These price-induced investments amount to 43% of . the measures targeted by the conservation program. As the control group invests in these measures over the course of the simulation, the "observed impact" declines over time.

An 11% annual increase in the real price of electricity is an extreme example which might characterize some utilities' difficult problems in the 1980s. But it is far beyond what most forecasters expect in the future. So I repeated the Figure 4 experiment with a lower rate of increase. (I chose a 2.7% annual increase based on one of the more difficult scenarios examined in the "1991 Scenario Analysis.") The new version of Figure 4 showed that the rate increase caused the observed impact to decline by a slight amount over the 10 year simulation. But the decline was imperceptible when compared to the benchmark simulation in Figure 3.

A final experiment with the first demonstration model assumes a general increase in the "base use" of electricity over time. This variable represents the electricity that would be used in the absence of any of the conservation measures shown in Figure 2. I assumed that the base use would increase at the annual rate of 2% for BOTH participants and nonparticipants. (The increase might be attributed to a general growth in affluence and more active use of the house. In practical terms, the increased use might take the form of higher comfort levels or more rooms in use.) The new experiment shows that the observed impact would also increase at 2% per year over the course of the simulation. This increase in "net savings" arises because conservation measures deliver greater savings when the home occupant is more active.

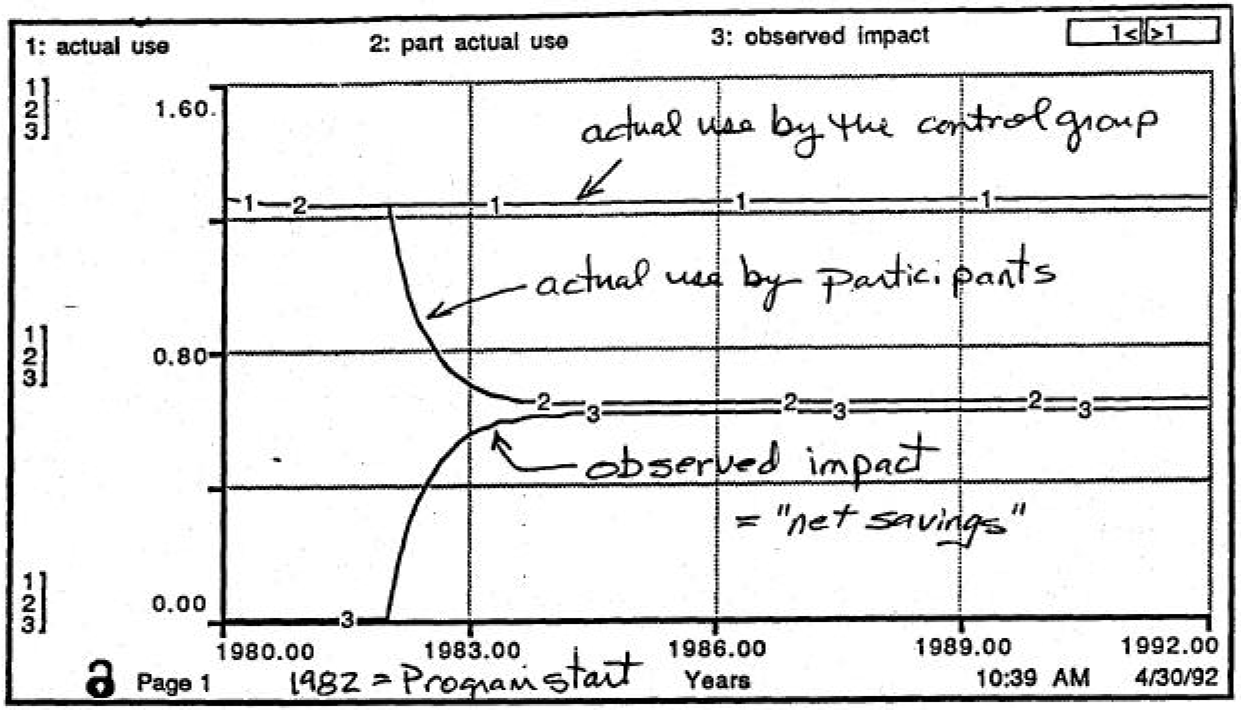


Figure 3. Benchmark Simulation Results from the First Demonstration

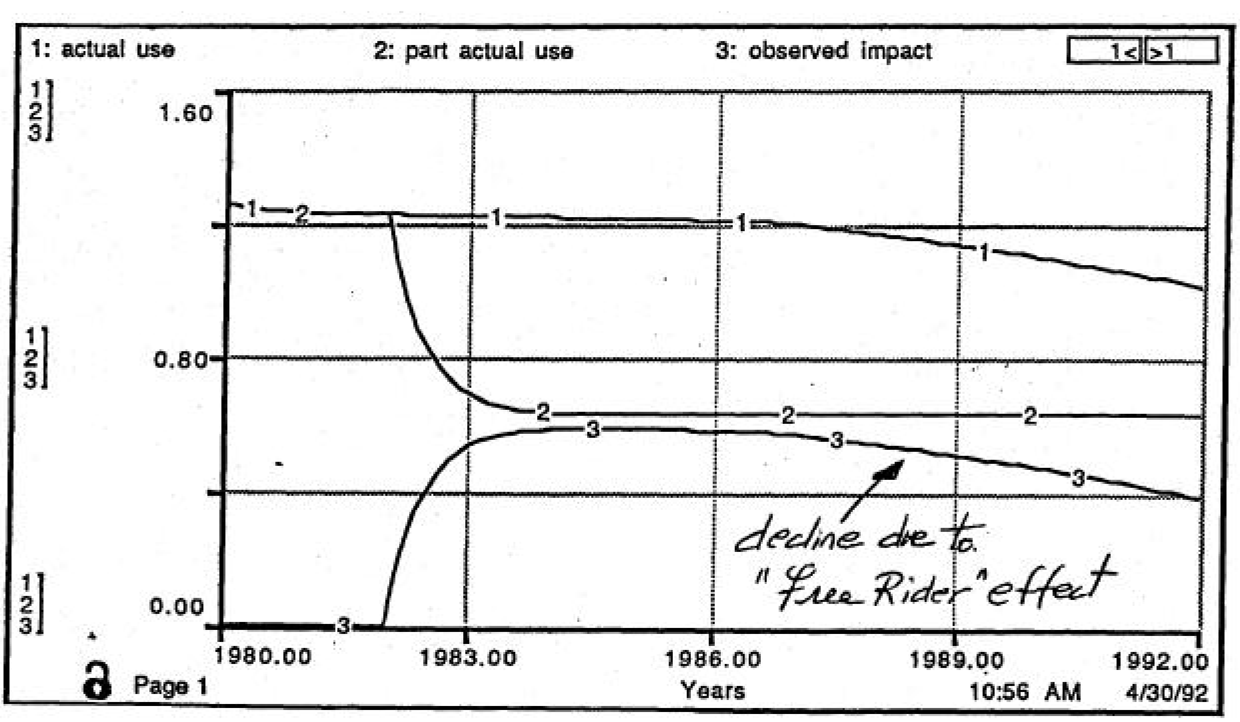


Figure 4. Simulated Decline in the "Observed Impact" when Electric Rates Increase Rapidly During the Study Interval

7. SECOND DEMONSTRATION:. ADD THE TAKE-EACK EFFECT

Fig. 5 shows a "map" of a second demo model in which the "take-back effect" is included in the participants sector. The new variables include the participants "electric bill," a "reference bill," the "bill ratio," an "instantaneous usage factor" followed by the "actual usage factor." The "instantaneous usage factor" is set at 1.0 when the participants' bill is within 20% of the "reference bill." But if the bill should become excessively high, the model assumes that participants will cut back on the usage of their electric equipment. Alternatively, if the bill should become quite, low, an increase in usage will occur. The maximum change in usage is set at plus-or-minus 20%. A 60% reduction in the electric bill is required to elicit the 20% increase in usage; 'a'60% increase in the electric bill is required to elicit the 20% reduction in usage. With the conservation program targeted to cut the electricity use in half, one would expect the participants to increase their usage by around 15%.

Figure 6 shows what conservation planners might expect for "observed impact" over the 12 year simulation. This diagram shows that net savings are around 12% short of the savings found in the previous demonstration. These are the "lost savings" from the "take back effect." But notice that the "observed impact" does not decline over time. The constant behavior shown in Figure 6 is due to the relatively short "response time" which links the "actual usage factor" and the "instantaneous usage factor" in Figure 5.

Figure 7 shows the "observed impact" over the 12 year simulation if the response time is lengthened from 1 year to 3 years. This longer response parameter assumes that participants do not react instantly to changes in their electric bill. The 3 year delay assumes that the participants monitor their bill over a three year period before adjusting their habits. Figure 7 shows that this longer response time causes a very slight decline in "observed impact" over time.

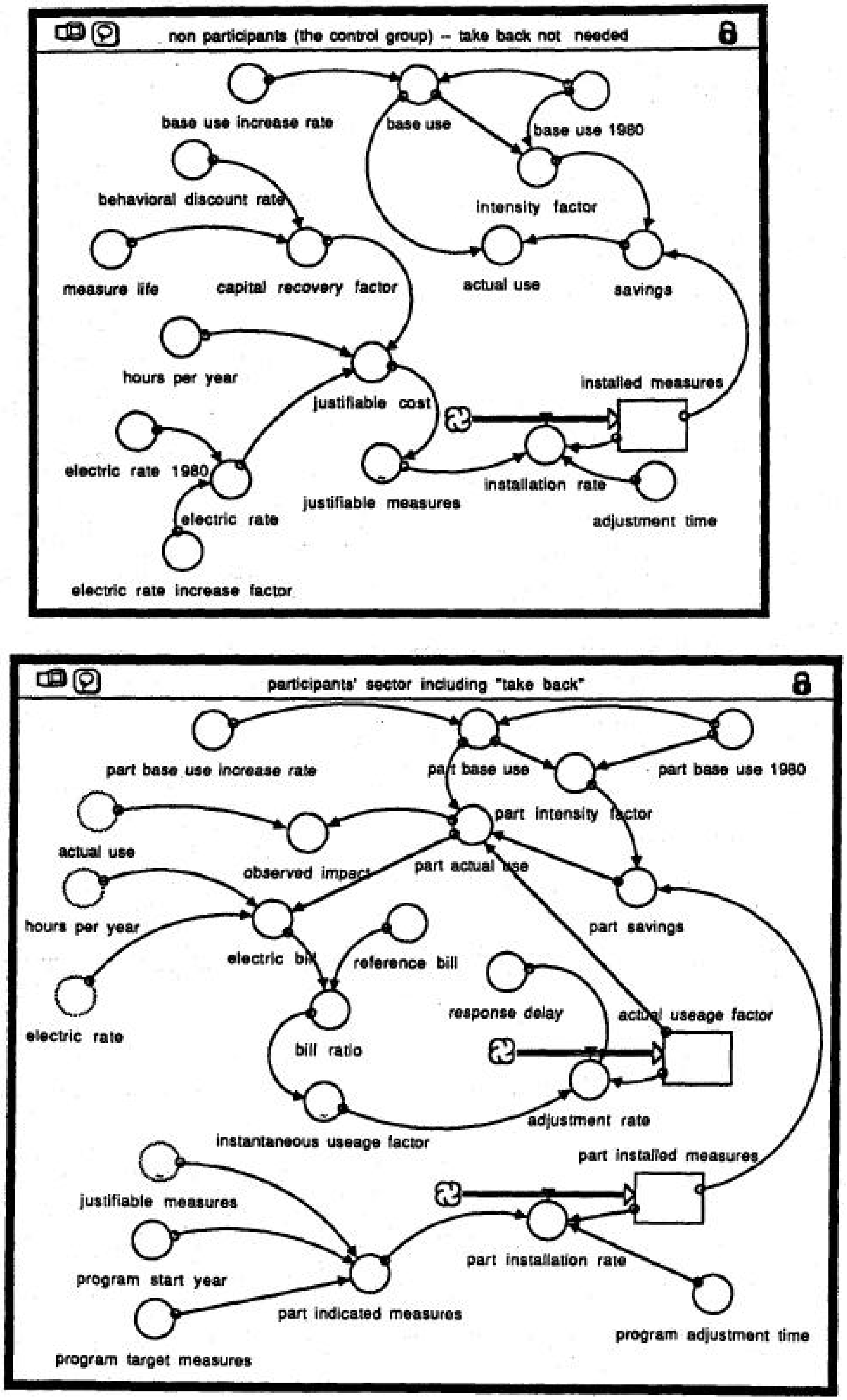


Figure 5. Structure of the Second Demonstration Model.

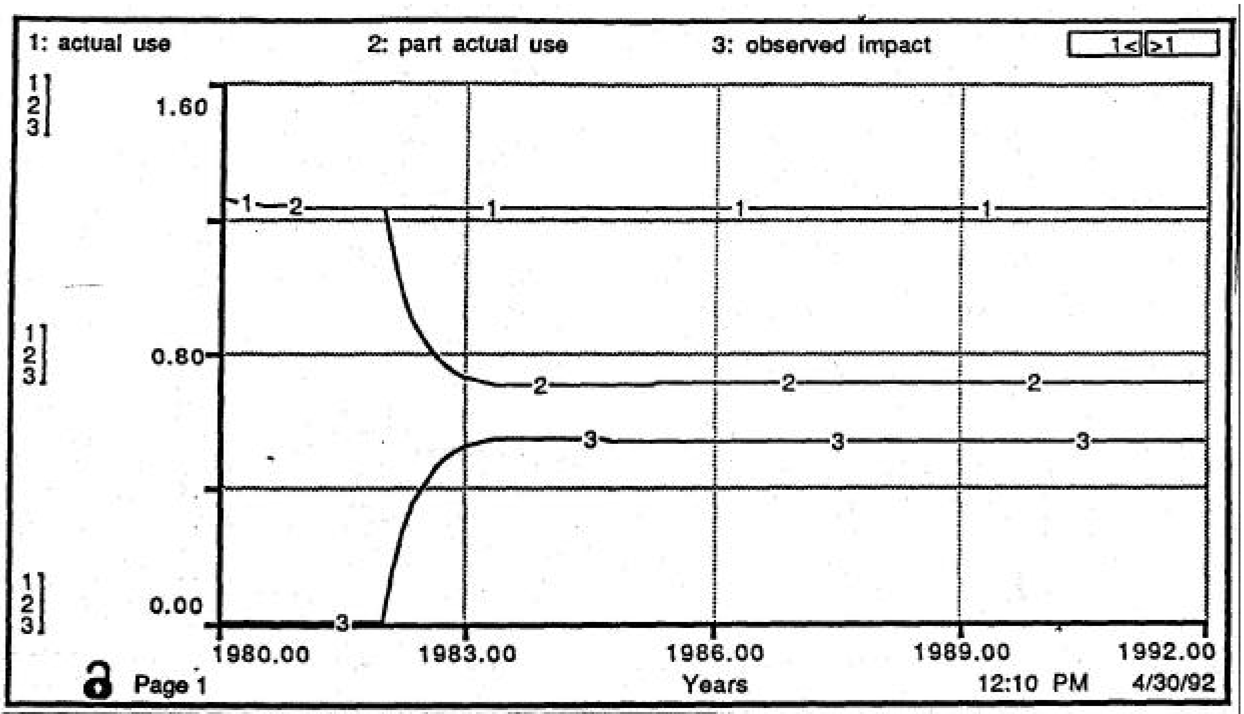


Figure 6. Simulated Impact of a 12% Loss of Savings due to the "Take Back Effect".

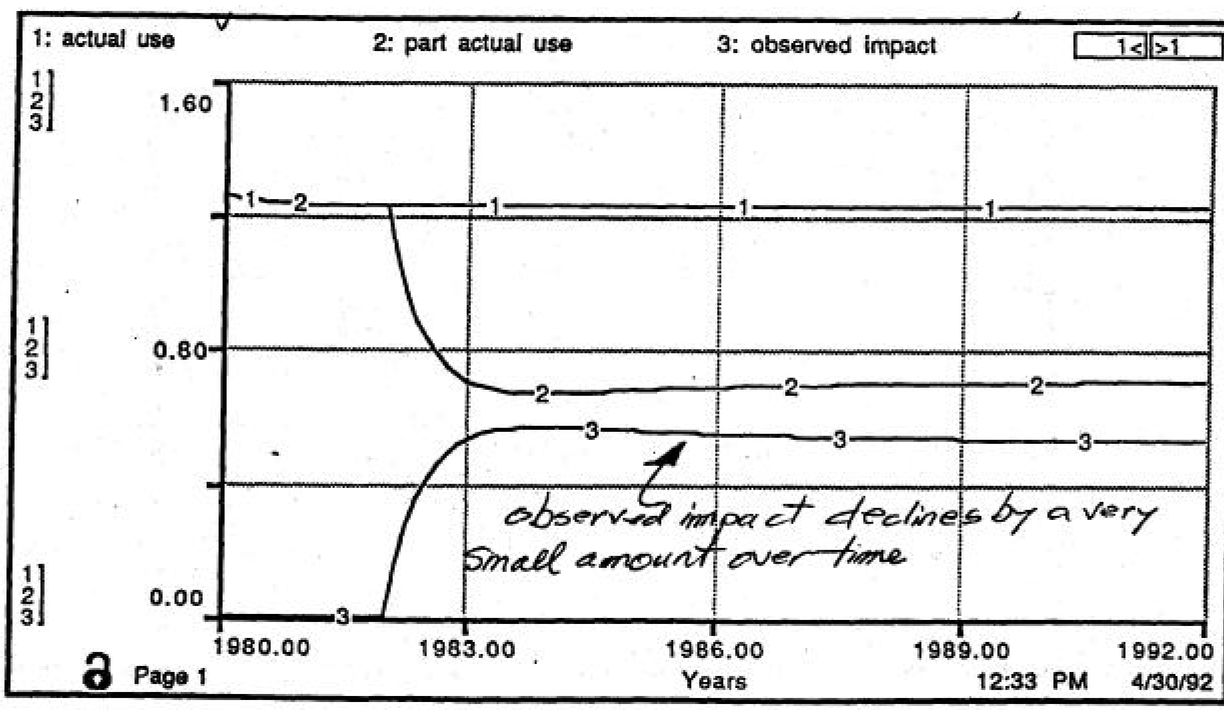


Figure 7. Simulated "Take Back Effect" with a Slower Response Time.

8. THIRD DEMONSTRATION: ADD THE FREE-DRIVER EFFECT

Figure 8 shows a change in the original model to incorporate a "free driver effect." This effect is explained by William Saxonis in Chapter 8 of the evaluation handbook noted in Table I (p. 132):

> The opposite of a free rider is a free driver. A free driver contributes to the goals of the program (e.g. reduce energy consumption) but is not formally a program participant. A free driver is affected by the program either through a conscious awareness of the program or because of program-induced changes in the marketplace. Free drivers require evaluators that use comparison groups to consider whether the comparison group is actually taking the conservation actions because of the program.

The Fig. 8 "map" introduces a new variable called "savings required to impress the control group." The "participants,' savings" are compared to this threshold level to determine the fraction of the control group' s members which will shift to a "low discount rate." The "behavioral discount rate" is a weighted average of the "regular discount rate" (set at 35 %/year) and a "low rate" said to characterize the nonparticipants who have become more impressed by (or less afraid of) the conservation measures.

Figure 9 shows the simulated impact on "observed impact" when the conservation program causes the control group to switch to a "low discount rate" of 20% quickly after the participants install the targeted measures. (The 20%/year value was taken from the low growth scenario in the Council's 1986 plan.) Figure 9 shows that this shift in control group attitudes causes the "observed impact" to be somewhat less than in the first demonstration. Figure 9 also shows that the "observed impact" is, constant over time since the control group's reaction occurs quite quickly after 1982.

Figure 10 repeats the "free driver" simulation with the assumption that the control group will be "driven" to the unusually low discount rate of 7 %/year by the impressive savings achieved by the participants. With this extreme assumption, the "observed impact" is much smaller, but it still remains constant over time.

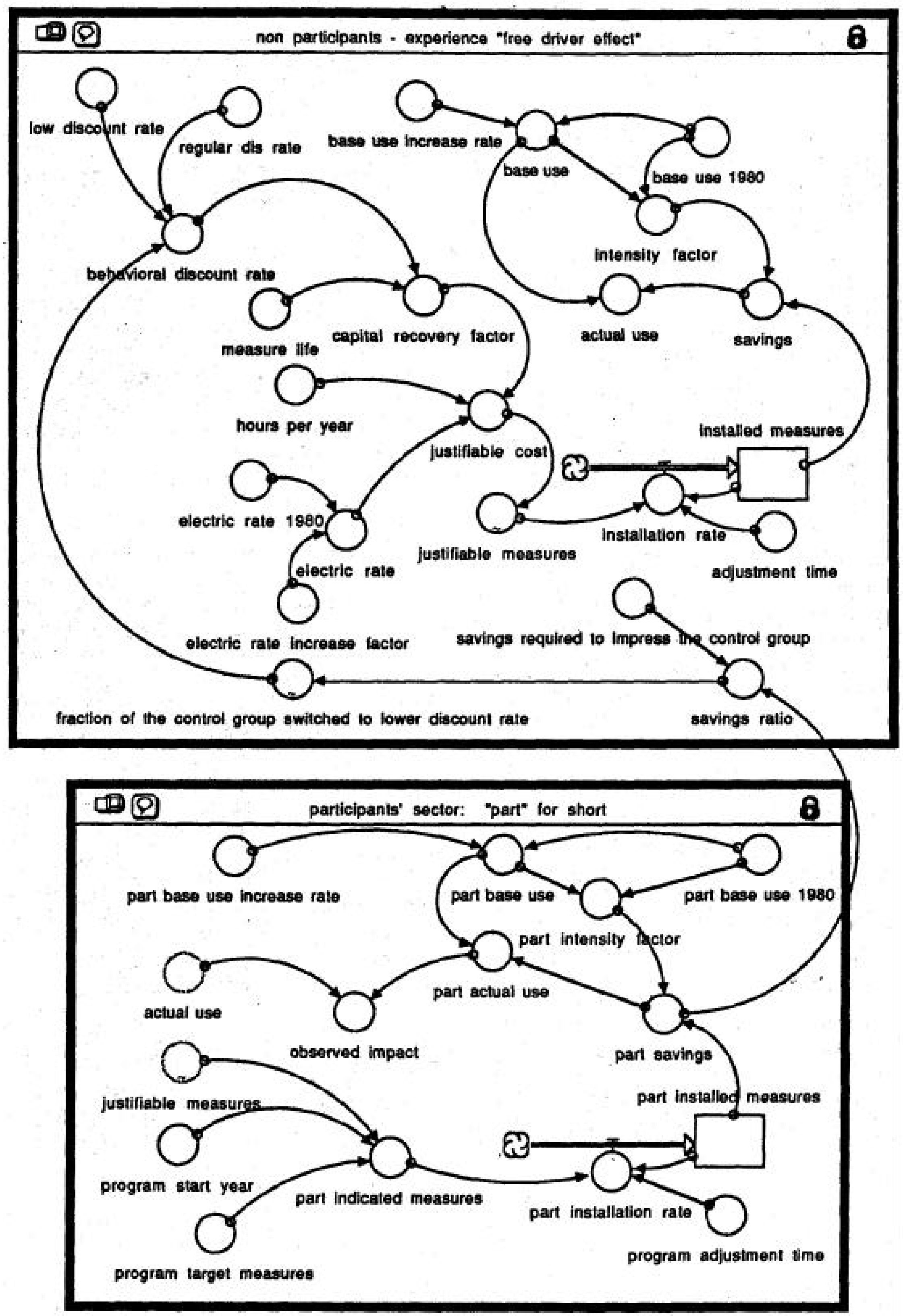


Figure 8. Structure of the Third Demonstration Model.

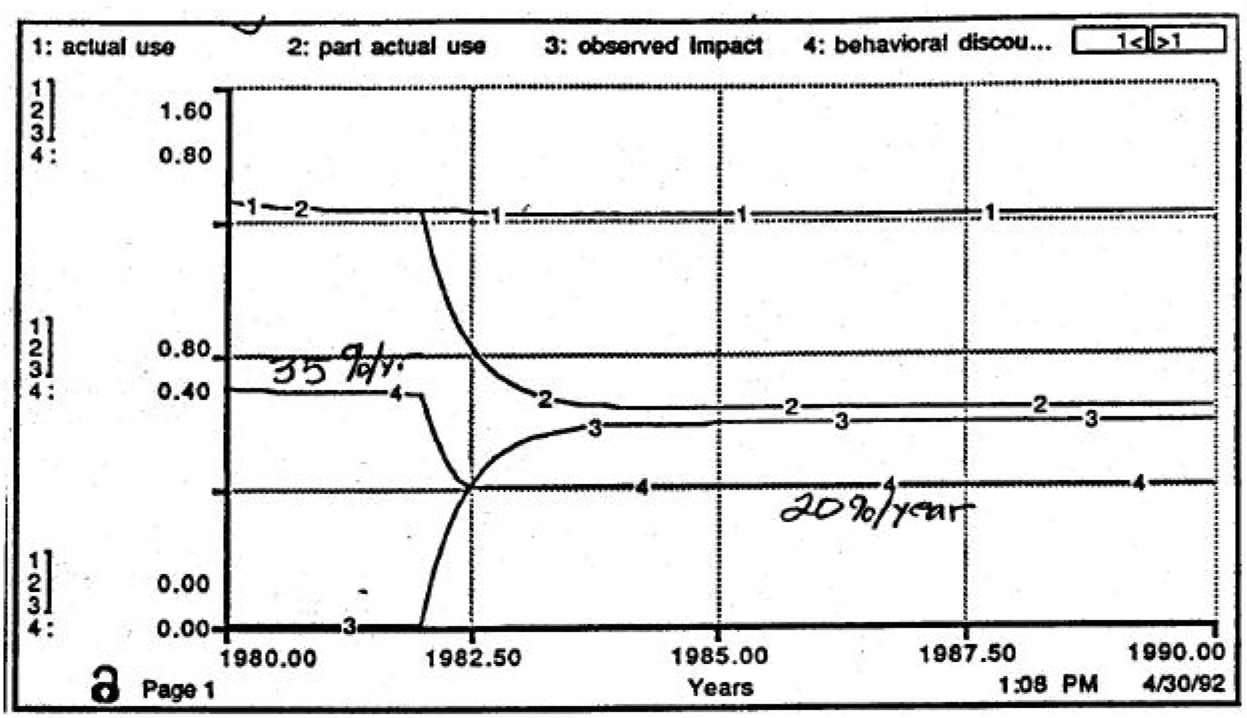


Figure 9. Simulated Impact of the "Free Driver Effect" When the Control Group Switches to the Low Discount Rate of 20% per year.

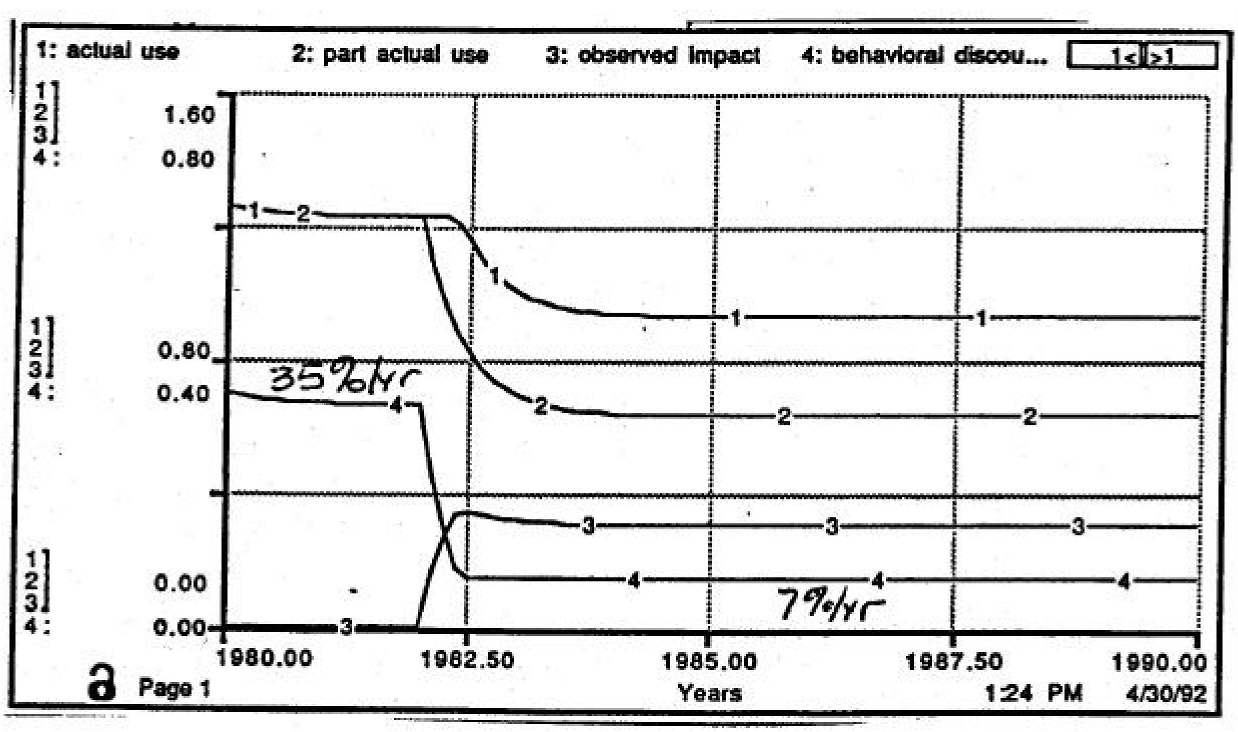


Figure 10. Simulated Impact of the "Free Driver Effect" When the Control Group Switches to the Low Discount Rate of 7% per year.

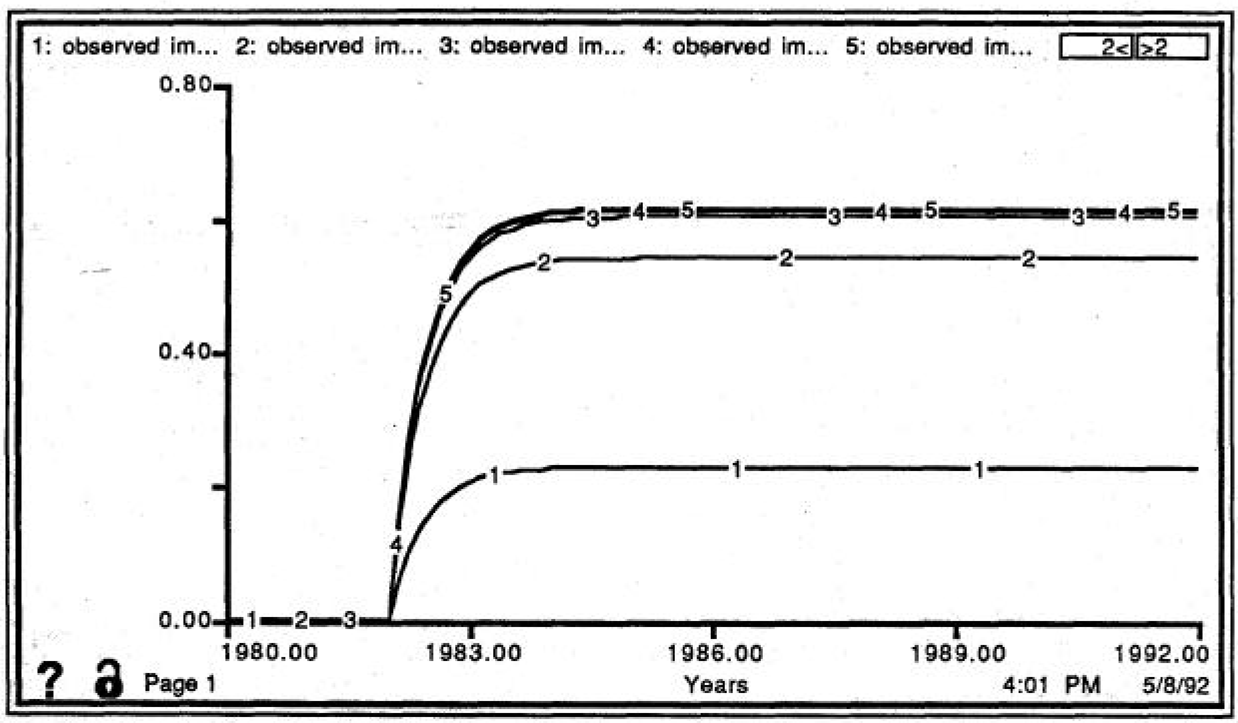
9. SENSITIVITY ANALYSIS OF THE FIRST DEMONSTRATION

The simulations with these three demonstration models show only one example of a perceptible decline in "observed impact" over time. That example appears in Figure 4 where the both the control group and the participants face an 11%/year real increase in the price of electricity. This pattern is studied further in Figure 11 by invoking the "sensitivity analysis" capability of the I-Think software

Figure 11A shows the "observed impact" from a set of five simulations with no increase in the electric rate. The behavioral discount rate is set at 5%, 15%, 35%, 55% and 75%, a range of values sufficient to cover the many estimates appearing in the literature. All five charts in Figure 11A show that net savings will remain constant over time, regardless of the value assigned to the behavioral discount rate.

.Figure 11B shows a second set of 5 simulations with different values assigned to the behavioral discount rate. These simulations all assume that electric rates increase at 5%/year in real dollars. The five charts show that the only perceptible signs of declining impact appear in the 1st and 2nd runs with the quite low discount rates.

Figure 11C shows a third set of 5 simulations with the assumption that electric rates increase at the rate of 10%/yr in real dollars. The five charts show perceptible signs of decline impact appear in three of the five simulations.

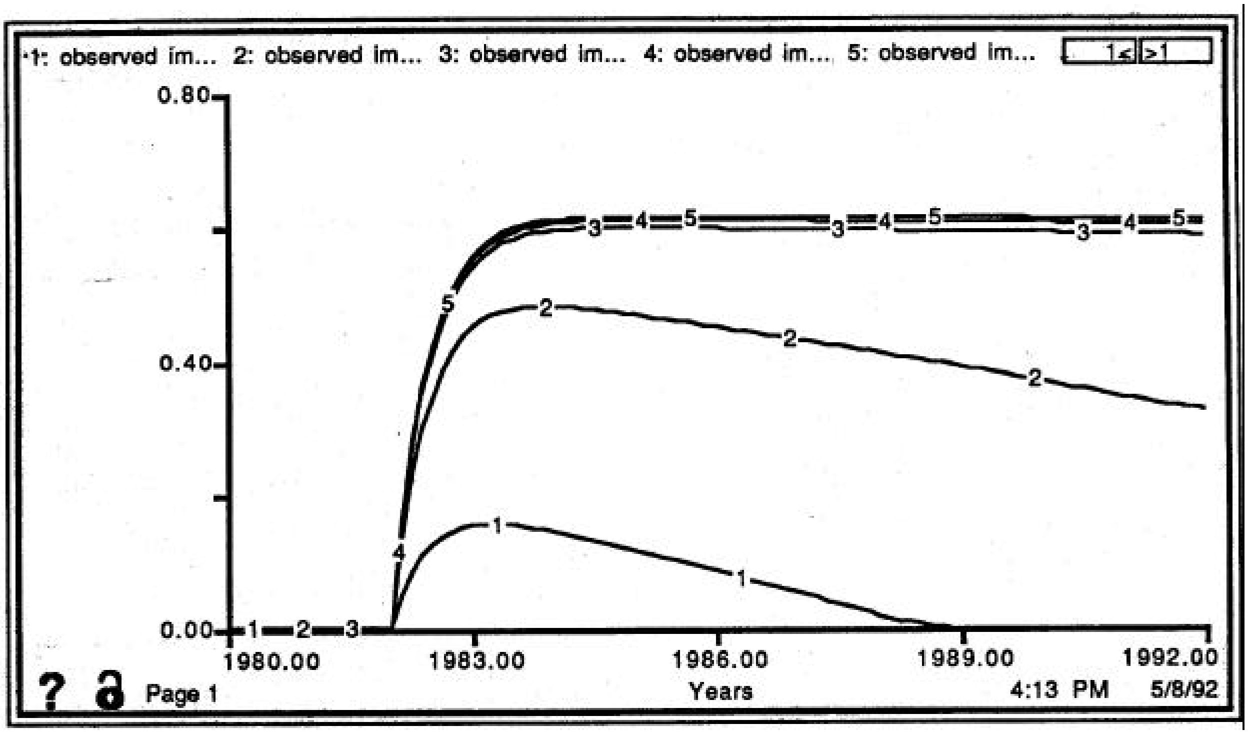


Setup #1 5/8/92 4:00 PM

Input Variables

Run	behavioral discou...	electric rate incr...
1	0.05	0.00
2	0.15	0.00
3	0.35	0.00
4	0.55	0.00
5	0.75	0.00

Figure 11A. Simulated Behavior of the "Observed Impact" in Five Simulations with the First Demonstration Model. These simulations assume that the electric rate will remain constant over the study interval.

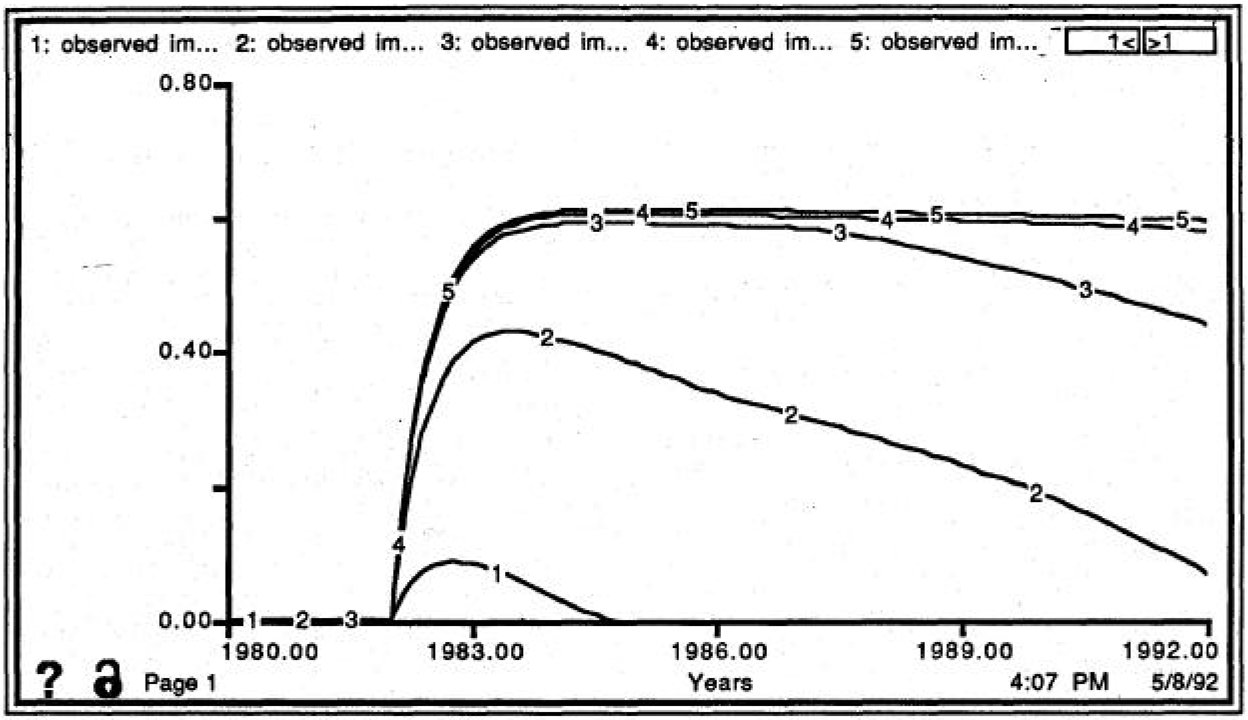


Setup #1 5/8/92 4:12 PM

Input Variables

Run	behavioral discou	electric rate incr
1	0.05	0.05
2	0.15	0.05
3	0.35	0.05
4	0.55	0.05
5	0.75	0.05

Figure 11B. Simulated Behavior of the "Observed Impact" in Five Simulations with the First Demonstration Model. These simulations assume that the electric rate will increase at 5% per year (in real terms) over the study interval.



Setup #1 5/8/92 4:07 PM

Input Variables

Run #	behavioral discou…	electric rate incr…
1	0.05	0.1
2	0.15	0.1
3	0.35	0.1
4	0.55	0.1
5	0.75	0.1

Figure 11C. Simulated Behavior of the "Observed Impact" in Five Simulations with the First Demonstration Model. These simulations assume that the electric rate will increase at 10% per year (in real terms) over the study interval.

10. DISCUSSION

This simple model was designed to communicate ideas about how conservation savings might change over time. So far, the model shows only one example with a decline in "net savings" over time. This decline arises when the control group is induced by *increasing* electric rates to invest in many of the measures targeted by the program. The other examples include changes in base use, a "take back effect" as well as a "free driver effect." Although these effects can change the amount of "net savings," they do NOT cause the "net savings" to decline over time.

This simple model was designed to start a discussion of what a useful model would look like in a synthetic data experiment. The experiment might include a model somewhat larger than the examples shown here. It would be run over a time period similar to a proposed longitudinal study, and its output could be altered to represent a synthetic form of the data that might emerge from such a study. I suggest that that the model could bypass the weather normalizing calculations. But it might be useful to simulate the loss in sample size over time due to attrition.

Such a model would help resource planners appreciate the findings from conservation evaluation studies. It could also help evaluators address what has been called the single, largest, unanswered question in demand-side management:

Will conservation savings persist overtime?

Appendix A. List of Equations for the First Demonstration Model

(page 1 of 2)

non participants sector: the control group

```
installed_measures(t) = installed_measures(t - dt) +
  (installation_rate) * dt
INIT installed_measures = 0
installation_rate = (justifiable_measures-
  installed_measures)/adjustment_time
actual_use = base_use-savings
adjustment_time = .5
base_use = base_use_1980*EXP(base_use_increase_rate*(TIME-1980))
base_use_1980 = 1.26
base_use_increase_rate = 0
behavioral_discount_rate = 0.35
capital_recovery_factor = -
  PMT(behavioral_discount_rate,measure_life,1,0)
electric_rate =
  electric_rate_1980*EXP(electric_rate_increase_factor*(TIME-
  1980))
electric_rate_1980 = 20
electric_rate_increase_factor = 0.05
hours_per_year = 8760
intensity_factor = base_use/base_use_1980
justifiable_cost =
  (electric_rate*hours_per_year/1000)/capital_recovery_factor

measure_life = 30
savings = installed_measures*intensity_factor
justifiable_measures = GRAPH(justifiable_cost)
(0.00, 0.00), (1000, 0.05), (2000, 0.3), (3000, 0.45), (4000,
  0.63), (5000, 0.7), (6000, 0.75), (7000, 0.8), (8000, 0.8),
  (9000, 0.8), (10000, 0.8)
```

participants' sector: "part" for short

```
part_installed_measures(t) = part_installed_measures(t - dt) +
  (part_installation_rate) * dt
INIT part_installed_measures = 0
part_installation_rate = (part_indicated_measures-
  part_installed_measures)/program_adjustment_time
electric_bill =
  part_actual_use*electric_rate*hours_per_year/1000
observed_impact = actual_use-part_actual_use
part_actual_use = part_base_use-part_savings
part_base_use =
  part_base_use_1980*EXP(part_base_use_increase_rate*(TIME-
  1980))
part_base_use_1980 = 1.26
part_base_use_increase_rate = 0

part_indicated_measures = IF(TIME<program_start_year) THEN
  justifiable_measures ELSE program_target_measures
part_intensity_factor = part_base_use/part_base_use_1980
part_savings = part_installed_measures*part_intensity_factor
program_adjustment_time = .5
program_start_year = 1982
program_target_measures = .63
```